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The Restart Effect and the Return to Full-time Stable Employment

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SUMMARY

In this paper we use nonparametric and semiparametric estimation procedures to analyse whether the restart programme in the UK has had any effect on reducing unemployment duration. The restart programme consists of an interview of the long-term unemployed to counsel them on effective job search. The statistical results utilize experimental data in which a control group does not receive the restart interview. The results show that the programme has had a significant effect (for the treatment group) of reducing the duration of unemployment. However, if we distinguish between exits to 'any job' and to full-time jobs lasting at least 3 months we find that the treatment group is no different from the control group in exits to 'stable jobs'.

Keywords: COX DURATION MODEL WITH FLEXIBLE BASE-LINE HAZARD; DURATION OF UNEMPLOYMENT; EXPERIMENTAL STATISTICAL POLICY EVALUATION; RESTART PROGRAMME

1. INTRODUCTION

Finding policies which are effective in securing transitions to full-time stable jobs for the long-term unemployed is a central part of the UK unemployment problem. Since a high proportion of the long-term unemployed are itinerant non-workers whose labour market history is characterized by long periods of unemployment punctuated only by occasional spells of work in temporary or part-time jobs, policy measures like the restart programme, which may help them to break out of the cycle of recurrent unemployment, could be very important. The question addressed in this study is whether the programme has succeeded in helping the long-term unemployed to secure full-time 'stable' jobs or whether instead it has been mainly effective in pushing people off the unemployment register, or into training programmes or part-time and temporary jobs. Such a distinction is important since it is more likely that securing a regular job will end the dependence of the unemployed person on state welfare payments.

The problem of repeated spells of unemployment has been extensively examined by many other researchers (see for example Heckman and Borjas (1980)). The link between the likelihood of unemployment and job types has also been recognized in 'dual labour market theories' (Dickens and Lang, 1985). We seek to analyse this dichotomy further by distinguishing between types of job obtained following a spell of unemployment. In particular we examine exits out of unemployment into 'any job' and exits into 'stable jobs' where the latter concentrates only on exits to jobs which

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are full time and last at least 3 months. If the restart initiative simply moves people off the unemployment register into a short-term alternative, so that they return to unemployment a few months later, then this may be an important indictment of the restart programme.

Although studies of labour market policies in the USA are increasingly using experimental data (LaLonde, 1985; Johnson and Klepinger, 1994) most empirical statistical studies in UK social science, including those concerned with evaluating government programmes, are non-experimental. (The exception is Royston (1983, 1984).) As a consequence they have encountered problems with non-random selection into such programmes. The resulting sample selection bias can severely contaminate estimates of the programme's effectiveness. The use of controlled experiments has been advocated as a means of overcoming this problem. Our study evaluates the restart programme by using data on the long-term unemployed based on a controlled experiment in which a purely random group of workers was excluded from the restart interview. It is the presence of such a control group that allows us to obtain unbiased estimates of the 'treatment effect' of the restart programme in altering the unemployment patterns of participants.

The restart programme was introduced by the government in April 1987 to review the position of people experiencing long-term unemployment. This section describes how the system worked at the time that our data were collected. The programme consisted of a set of six-monthly meetings between the unemployed individual and a counsellor. During this interview the counsellor assessed the claimant's recent unemployment history and offered advice on benefits, search behaviour, training courses and in some instances initiated direct contact with employers. The main aim of the restart process was to help the unemployed to return to full-time stable employment thereby reducing the amount of time that people spend unemployed and hence reducing their claims of unemployment benefit. The programme also aimed to reduce welfare claims by exposing those not available for work and those not making appropriate efforts to find employment. It is this threat component which may induce people to accept temporary or part-time jobs simply to escape the attentions of the restart officer.

The process begins when the restart office sends a letter to each individual who is approaching an unbroken period of 6 months claiming unemployment benefit. The letter requests that the individual attends an interview at a stated date and time. Interviews take place in Employment Service Job Centres and last approximately 15–25 minutes. At the time that our data were collected the benefit office, where claims were made and payments received, was distinct from the Employment Service Job Centre where restart interviews took place and where information on job vacancies and training programmes was provided. In some instances individuals were excused attendance at the restart interview mainly because they had already obtained a job, or a place on a training programme or had withdrawn their benefit claim. On completing the interview the restart counsellors then recommended a course of action designed to help that individual in their job search. Attendance at the restart interview was mandatory, in that it was a condition of receiving unemployment benefits that claimants attend an employment interview when asked to do so.

Recent research has suggested that the restart programme is important in making people leave unemployment more quickly than they would otherwise do without the programme (Dolton and O'Neill, 1996). However, that work did not distinguish between the type of jobs which people exited to. This paper investigates such a distinction by considering two definitions of the exit state to explore whether the receipt of a restart interview has a different effect for exit to any job whether full or part time (however little time it lasts) and exit to a stable job.

In the context of duration data it is also important to distinguish the point in time at which the two groups become distinct, if and when they do. In Section 3 we utilize the nonparametric methods of Dinse *et al.* (1993) to facilitate this. In Section 4 we estimate a Cox proportional hazards model to investigate further the difference between the prospects of the control and treatment groups in finding stable jobs. In these estimations we control for differences in human capital and personal characteristics, explore the possible presence of unobserved heterogeneity and estimate a Meyer (1990) type of flexible base-line hazard.

The main finding of the paper is that, although the restart programme has succeeded in moving people off the unemployment register by inducing them to take any job, it has been less successful in terms of moving people into long-term stable employment.

2. DATA

In 1989 the Policy Studies Institute was commissioned by the Employment Service to evaluate the effect of the restart initiative (White and Lakey, 1992). This study identified a sample of individuals approaching their sixth month of unemployment in the period March–July 1989 who were eligible for a restart interview. A random sample of 8925 of these individuals was chosen to take part in the study. Individuals were retained in the sample even if they subsequently did not attend a scheduled interview; therefore the sample is one of the inflow to the restart programme and not the outflow from it. Every Employment Service office throughout Britain was contacted while constructing the sample to eliminate regional biases. Individuals were selected for the sample from the inflow lists on the basis of the last three digits of their national insurance (NI) numbers. An NI digit sequence known to result in a random 5% sample was used to construct our data. In this sample a control group of 582 people was randomly chosen, again by means of previously specified NI digit sequences. Members of the control group, although eligible for an interview, were not asked to attend the initial restart interview. For each individual in the sample, data were collected on personal characteristics such as sex and age as well as information on the restart interview and outcome. About 6 months after the restart interview, the survey organization, Social and Community Planning Research, conducted a survey of these individuals in which detailed information was obtained on subsequent work history, personal characteristics, the restart interview, previous employment history, job search behaviour and benefit income. Of the original sample, 5200 individuals completed this survey, which was conducted between September and October 1989. Of these, 3242 also entered the second survey carried out approximately 6 months after the first. For these individuals information from the second survey was used to extend their unemployment histories. Approximately half of the non-responses resulted from an inability to contact the individual because of invalid address records or death, whereas the other half refused to take part in the survey. Estimates of a probit equation determining survey participation suggest that the decision to participate was independent of control group status. Of the 5200 respondents, 4552 reported valid data on the variables of interest of which 286 were members of the control group.

As already explained the data that we use are virtually unique in having an experimental control group, who were chosen randomly to be excluded from the restart interview. Since some of our sample never exit unemployment the techniques which we use must take into account the censored nature of the dependent variable. It was also possible to link an individual's geographical location to data on labour market conditions in an individual's travel-to-work area via the national on-line manpower information system. This allows us to obtain monthly data on local labour market conditions dating back to August 1985. A description of the variables used in this study along with summary statistics are presented in Appendix A and Table 3 there.

3. NONPARAMETRIC DESCRIPTION OF THE DATA

In this section we examine whether durations of unemployment for those in the treatment group are distinct from those in the control group for our various definitions of what constitutes a completed spell in unemployment. From the outset of our analysis we should be clear about the nature of the exit state which defines the end of an unemployment spell. Our central concern is to distinguish between the any job exit, the stable job exit and the censoring process used in the construction of these durations.

The first obvious query is why we choose to define a stable job as a job which lasts at least 3 months. To some extent the choice of the time interval over which a job is judged to be stable is arbitrary. However, our choice is based on two commonly used criteria: the first is that the most generous condition for termination by either party in a job hiring contract is for the job to end on 3 months' notice (from either party); the second is that current unemployment benefit eligibility rules allow an unemployed person to try out a job (in the 'employment on trial' scheme) for up to 3 months during which they can quit without losing their entitlement to benefit. To examine the robustness of our findings to our choice of 3 months we also carried out all our analysis by using a 4-month definition of stable job, which only strengthened our findings.

We should also be clear about the precise form of the censoring process. It is possible to characterize the outcomes of exit from unemployment in these data as either entry to a job, entry to a training programme or signing off claiming unemployment benefit. In computing durations to any job or a stable job we must decide how to treat those exiting unemployment for other reasons. For these durations we consider that those who exited to 'signing off' are censored at the point that they declared themselves to be no longer looking for a job, by signing off. Since many of these people may not want to work or be available for work after signing off it seemed unreasonable to judge the restart initiative on its ability to place such individuals. For those who exited to training we subtracted their time spent in training from their duration of unemployment on the grounds that they were not still unemployed and were taking active steps to find a job. However, since they had not yet found a job they could not be judged to have a completed unemployment spell. Furthermore, in constructing duration to a stable job, exits to part-time jobs or jobs lasting less than 3 months are included as time spent unemployed. To examine the sensitivity of our results to these assumptions we repeated all the statistical analysis which follows for a data set which excluded those exiting to signing off and a data set which counted training time (as well as time spent signing off) as time in unemployment. All our results are robust to these changes.

Of central concern in this study is the comparison of the survival distributions of unemployment times for two groups: the treatment group who had a restart interview and a control group who did not have the interview. Survival in this context is the time until exit from unemployment and is recorded in months from the date at which the restart interview was administered. To examine this we construct nonparametric estimates of the survival curves by using the methods of Kaplan and Meier (1958). We also use the Mantel test statistic (Mantel, 1966; Cox, 1972) to establish whether there is an overall difference in the two curves.

Fig. 1 plots the survival curves for the terminal event of entering any job including part-time jobs or temporary jobs. Since all in our sample are eligible for a restart interview they all have experienced at least 6 months unemployment. In all our analysis we therefore measure duration of unemployment in months from the date that the individual is first identified as being eligible for a restart interview. From Fig. 1 we see that the survival curve for the control group lies everywhere above the corresponding curve for the treatment group. Not surprisingly, the Mantel statistic supports this visual interpretation with a value on the statistic of 2.78 which is significant at the 1% level. The corresponding figure for exits to a full-time job lasting at least 3 months is provided in Fig. 2, which clearly shows that for exits to a stable job the survival curves for the two groups are more similar. In this case the Mantel statistic of 0.96 suggests that the two groups are only different at the 34% significance level, i.e. they are not distinct over the whole range of T. This result suggests that there is no significant difference between the control and the treatment group if we use the exit state of entrance to a stable job.

So far we have asked whether the two groups differ from one another over the entire range of T. However, a more informative approach may be to examine the range of time over which the control and treatment group are significantly different

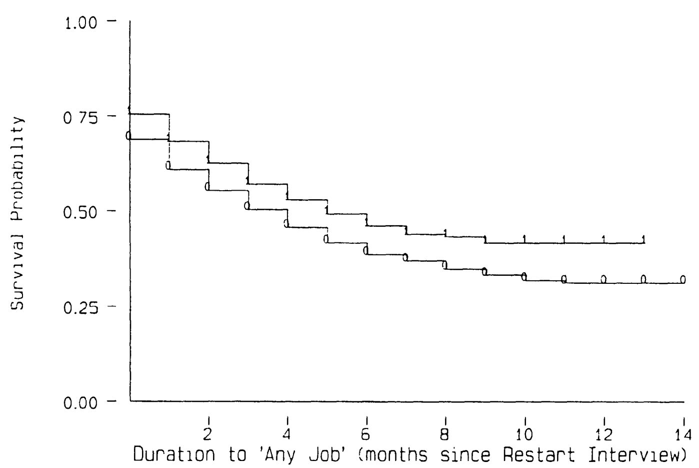

Fig. 1. Kaplan–Meier survival curves for duration to any job: 0, treatment group; 1, control group

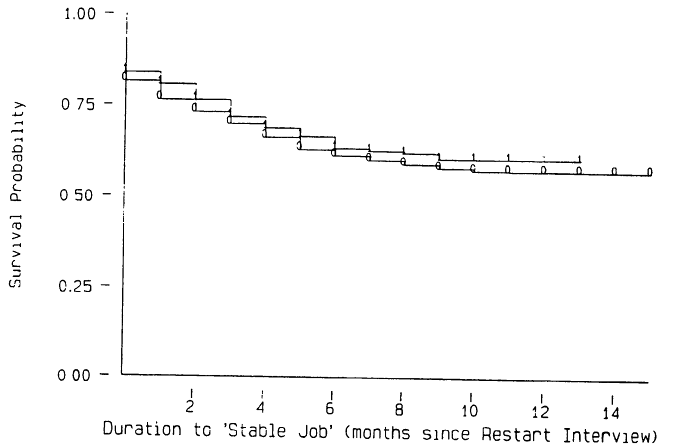

Fig. 2. Kaplan–Meier survival curves for duration to stable job: 0, treatment group; 1, control group

from one another. The method of Dinse *et al.* (1993) is appropriate to answer this question. To present their statistic we need to introduce some notation.

Let the treatment and control groups be group 0 and group 1 respectively. Let T_k be a positive random variable representing time until exit from unemployment, and let $S_k(t) = P(T_k > t)$ denote the survivor function for people in group k ($k = 0, 1$). For group k, let d_{jk} represent the number of exits from unemployment to a job at time t_j and let c_{jk} represent the number of censored observations in the interval of the month. Similarly, let r_{jk} denote the number of people at risk of exit at time t_j in group k:

$$r_{jk} = \sum_{i=j}^{J} (c_{ik} + d_{ik}). \tag{1}$$

Let $h_{jk} = P(T_k > t_j | T_k > t_{j-1})$ denote the probability of surviving past time t_j in group k, conditional on having survived past time t_{j-1} where $t_0 = 0$.

Kaplan and Meier (1958) derived the maximum likelihood estimates of h_{jk} and $S_k(t)$, which are

$$\hat{h}_{jk} = (r_{ik} - d_{ik})/r_{ik}, \tag{2}$$

$$\hat{S}_k(t) = \prod_{\{i:t_i \leqslant t\}} \hat{h}_{jk} \tag{3}$$

respectively.

The variance of $\hat{S}_k(t)$ is usually estimated by using Greenwood's formula:

$$\hat{V}_k(t) = \hat{S}_k(t)^2 \sum_{t_i \leqslant t} \frac{d_{ik}}{r_{ik}(r_{ik} - d_{ik})}. \tag{4}$$

If we approximate the asymptotic distribution of $\hat{S}_k(t)$ by that of a normal random variable with mean $S_k(t)$ and variance $V_k(t)$, then using the intersection–union

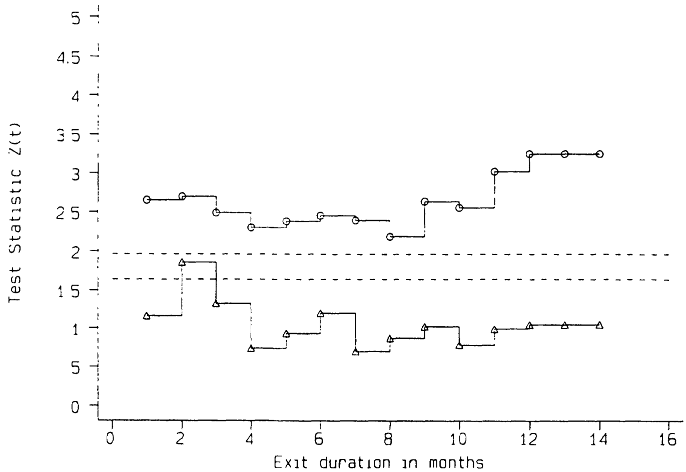

Fig. 3. $Z(t)$ *versus* T for exit to any job (○) and to stable job (△)

principle Dinse *et al.* (1993) suggested the following test to distinguish between two survivor functions at any specific point:

$$Z(t)=\frac{\hat{S}_0(t)-\hat{S}_1(t)}{\surd\{\hat{V}_0(t)+\hat{V}_1(t)\}}. \tag{5}$$

This statistic is plotted in Fig. 3 for each value of T for the alternative definitions of the exit state along with its 5% and 2.5% critical values (1.96 and 1.64) on a one-tail test. We can see quite clearly that for completing a spell by exit to any job the control and treatment groups are significantly different at both the 5% and the 2.5% levels for all values of T. In contrast the same plot for the exit to a stable job shows that the control and treatment groups can only be considered to be statistically different at the 5% level when $T=2$. To the extent that the restart programme has an effect on the probability of exit to a stable job it occurs within 2 months of the interview at the 6-month stage but not thereafter.

4. ECONOMETRIC SPECIFICATION AND ESTIMATION

To extend this analysis we formally model the duration of unemployment in an attempt to establish the existence of a possible restart effect. For this we use a Cox proportional hazard specification with unrestricted base-line hazard

$$h_i(t)=h_0(t)\exp(X_i'\beta) \tag{6}$$

where $h_0(t)$ is the base-line hazard at time t (where in our case t is the elapsed time from the date that individuals are first identified as eligible for a restart interview), X_i is a vector of explanatory variables for individual i, including a variable indicating treatment status, and β is a vector of unknown parameters. We have dropped the k-subscript from the previous section as we now include a dummy regressor variable for control group status.

Following Meyer (1990) we estimate jointly the base-line hazard and the coefficients on the covariates. This semiparametric estimation procedure has the advantage of preventing inconsistent estimation of the covariate coefficients due to a misspecified base-line hazard and simultaneously providing a semiparametric estimate of the base-line hazard. For interval data of the type analysed here, where unemployment durations are measured in complete months, the probability of observing a complete (uncensored) duration of t_i months for individual i with a vector of characteristics X_i is

$$P(t_i \leqslant T < t_i + 1) = P\left\{ \int_0^{t_i} h_i(u)\,\mathrm{d}u \leqslant \int_0^{T} h_i(u)\,\mathrm{d}u < \int_0^{t_i+1} h_i(u)\,\mathrm{d}u \right\}$$

$$= \exp\left\{ -\exp(X_i'\beta) \sum_{s=1}^{t_i} \int_{s-1}^{s} h_0(u)\,\mathrm{d}u \right\} - \exp\left\{ -\exp(X_i'\beta) \sum_{s=1}^{t_i+1} \int_{s-1}^{s} h_0(u)\,\mathrm{d}u \right\} \tag{7}$$

$$= [1 - \exp\{-\exp(X_i'\beta)\,\gamma(t_i + 1)\}]\,\exp\left\{ -\exp(X_i'\beta) \sum_{s=1}^{t_i} \gamma(s) \right\} \tag{8}$$

where

$$\gamma(s) = \int_{s-1}^{s} h_0(u)\,\mathrm{d}u,$$

T is the actual (unobserved) duration and we use the fact that minus the logarithm of the integrated hazard function has (conditional on X_i) an extreme value distribution with distribution function $F(\epsilon) = \exp\{-\exp(-\epsilon)\}$. Note that the value of X_i is assumed to be constant inside each $[s-1, s)$ interval. The first term in expression (8) measures the probability of an exit in the $[t_i, t_i + 1)$ interval given that the spell has lasted until t_i and is therefore the discrete (grouped) interval hazard rate. The second term shows the probability of staying unemployed at least until t_i, or the survival probability $P(T \geqslant t_i)$. This term will be the contribution to the likelihood function of individuals with right-censored spells of unemployment. For the sample of N individuals the likelihood can therefore be written as

$$\begin{aligned} L^1(h_0, \beta) &= \prod_{i=1}^{N} L_i(t_i, c_i) \\ &= \prod_{i=1}^{N} [1 - \exp\{-\exp(X_i'\beta)\,\gamma(t_i + 1)\}]^{c_i}\,\exp\left\{ -\exp(X_i'\beta) \sum_{s=1}^{t_i} \gamma(s) \right\} \end{aligned} \tag{9}$$

where c_i is the censoring indicator with $c_i = 1$ for a completed (uncensored) spell and $c_i = 0$ if the duration is right censored at t_i. Maximization of the log-likelihood, $\ln L$, with respect to h_0 (the $\gamma(s)$-terms) and β, under the constraint that the hazard pieces $\gamma(s)$ are non-negative, will provide us with consistent estimates of the base-line hazard pieces

$$\int_{s-1}^{s} h_0(u)\,\mathrm{d}u$$

and of the parameter vector β (see Meyer (1990) and Narendranathan and Stewart (1993)).

It is well known that omitted unobserved heterogeneity may lead to selection bias in the estimation of the base-line hazard and in the parameter estimates of the included explanatory variables. One approach to dealing with this problem is to introduce unobserved heterogeneity in the form of an omitted variable v_i in the heterogeneity term of the hazard function: $\exp(X_i'\beta + v_i)$, with v independent of X. Combining this heterogeneity term with the base-line hazard function provides us with the conditional hazard function (i.e. conditional on v). However, since v is unobservable we must first obtain the unconditional hazard function to carry out the estimation. To do this we calculate the joint density of t and v and then integrate over v. In this paper we assume that $\exp v$ has a gamma distribution with mean 1 (a normalization) and variance σ^2. The gamma distribution assumption is a practical convenience as there are relatively few tractable distributions which provide a closed form solution for the unconditional hazard function. An alternative would be to use the Heckman and Singer (1984) nonparametric approach to modelling unobserved heterogeneity. Under these assumptions the likelihood is given by

$$L^2(h_0, \beta, \sigma^2) = \prod_{i=1}^{N} \left\{1 + \sigma^2 \exp(X_i'\beta) \sum_{s=1}^{t_i} \gamma(s)\right\}^{-1/\sigma^2} - c_i \left\{1 + \sigma^2 \exp(X_i'\beta) \sum_{s=1}^{t_i+1} \gamma(s)\right\}^{-1/\sigma^2}. \tag{10}$$

To estimate the effect of the restart programme on duration of unemployment we estimate directly the specification in equation (10) for the exit definitions described above, assuming that the treatment effect can be captured by including a dummy variable for control group membership among the regressors of the hazard function. Such models, known as 'frailty models' in the biostatistics literature, could also be estimated in the variance components form by using multilevel methods (see Goldstein (1995), chapter 9).

5. ECONOMETRIC ESTIMATES FROM MODEL OF UNEMPLOYMENT DURATION

The results of our estimation for the any job and the stable job distinctions on the types of exit are presented in Table 1, with the estimates of the underlying base-line hazard presented in Table 2. The estimates in Table 2 highlight the importance of using the semiparametric approach in estimating the base-line hazard in that the spikes occurring in the hazard make parametric estimation difficult. In particular we see that the base-line hazard has two distinct spikes, the first occurring at 1 month (7 months total unemployment) and the second at 6 months (12 months total unemployment). These correspond closely to the timing of the restart interviews and suggest that the receipt of such interviews increases the probability of leaving unemployment. The hazard function declines rapidly after 6 months (12 months total unemployment) suggesting that individuals unemployed for a year or more have little chance of obtaining employment.

The choice of regressors used in the estimation is consistent with those used in previous studies and consists of variables which can be thought of as affecting the individual's probability of receiving a job offer, as well as those that affect the probability that an offer, once received, will be accepted. Of particular interest for our analysis is the coefficient on the control variable, which takes the value 1 if the individual was a member of the control group excluded from the restart process and

TABLE 1

Estimates from the proportional hazards model, nonparametric base-line hazard and gamma unobserved heterogeneity

Variable	*Any job*		*Stable job*	
	Estimate	*Standard error*	*Estimate*	*Standard error*
Control	−0.219†	0.089	−0.117	0.151
Benefit entitlement	−0.097†	0.007	−0.170†	0.015
Sex	0.170†	0.052	0.583†	0.102
Age25	0.426†	0.071	0.818†	0.130
Age35	0.381†	0.079	0.833†	0.149
Age45	−0.118†	0.076	0.046	0.135
Age55	−0.672†	0.090	−0.917†	0.164
Married	2.453†	0.179	4.228†	0.391
Divorced	0.901†	0.092	1.308†	0.177
Dependent kids	0.843†	0.069	1.275†	0.136
Toddlers	−0.335†	0.055	−0.288†	0.098
Local unemployment	1.801†	0.367	2.573†	0.701
Inner city	−0.098	0.057	−0.132	0.099
Race	0.093	0.155	0.215	0.275
Education	0.156†	0.044	0.152	0.079
Driver	0.257†	0.046	0.229†	0.080
Local authority house	0.572†	0.073	1.176†	0.144
Rent house	0.757†	0.092	1.391†	0.182
Other house	−0.374	0.197	−0.260	0.314
Past unemployment	−0.461†	0.089	−0.735†	0.162
Active partner	−3.054†	0.246	−5.870†	0.556
σ^2	0.014	0.076	1.723†	0.417
N	4552		4552	
Log-likelihood	−6619.90		−5374.88	

†Significant at the 5% level.

TABLE 2

Base-line hazard estimates†

Duration	*Any job*		*Stable job*	
	Hazard	*Standard error*	*Hazard*	*Standard error*
1	0.331747	0.009812	0.207357	0.011687
2	0.121678	0.007928	0.092835	0.01171
3	0.103805	0.00835	0.078856	0.012333
4	0.111877	0.010301	0.094935	0.01683
5	0.116565	0.012233	0.118037	0.023475
6	0.120773	0.014487	0.125045	0.028279
7	0.101708	0.014	0.079038	0.020093
8	0.062473	0.011213	0.05214	0.015374
9	0.089599	0.016077	0.062374	0.019541
10	0.079821	0.017079	0.069863	0.023901
11	0.084183	0.023365	0.047254	0.021668
12	0.054258	0.027577	0.014206	0.014617
13	0	0	0	0
14	0	0	0	0
15	0	0	0	0
16	0	0	0	0

†Our programme returns zero estimates of the base-line between 13 and 16 months, since there are fewer observations at these durations and the conditional probability of exiting unemployment becomes arbitrarily small.

0 otherwise. The results support our earlier findings in that, for the specifications relating to the any job durations, individuals who attended a restart interview were significantly more likely to exit the state of unemployment than those in the control group who were excluded from the process. However, this is not so for the specification relating to a stable job. In this specification the coefficient on the control dummy is not significantly different from 0 at the 5% level.

Of the other variables in the model we see that increases in the level of unemployment benefits significantly reduce the probability of leaving unemployment. Age, poor local labour market conditions (local unemployment), previous unemployment (past unemployment), lack of formal qualifications (education $\equiv 0$) or a driver's licence (driver $\equiv 0$) and being female (sex $\equiv 0$) all have a significant negative effect on the probability of leaving unemployment. We also note that individuals who have working partners are significantly less likely to exit the state of unemployment, even after controlling for the reduction in benefits that this leads to. If we view non-labour income, in this case from one's partner, as a source of financing one's search we can interpret this effect in a similar fashion to the benefit effect in that such individuals are not under as much pressure to obtain work and thus can be more choosy in the type of jobs that they accept.

To control for unobserved heterogeneity we explicitly assume a gamma form for it by maximizing the likelihood given by L^2. The coefficient for the heterogeneity term is reported as the estimated σ^2 from equation (10). In the reported results in Table 1 we see that this heterogeneity parameter is statistically significant at the 5% level in the stable job equation. The parameter estimate is insignificant in the any job equation. It is not exactly clear what factors explain this result; however, it is possible that the lack of unobserved heterogeneity found in the any job exit is due to the fact that most unemployed people could be offered a job temporarily when the employers know that they can get rid of employees if they prove unsuitable.

To examine the robustness of our parameter estimates we have also estimated the models separately for the control and treatment groups. We found significant unobserved heterogeneity within the treatment group but were unable to estimate the heterogeneity parameter for the control group. This may be due to the small sample size in the control group. In both cases the remaining parameter estimates were similar to those presented in Table 1.

6. CONCLUSION

This paper has examined the effect of the restart programme on the probability of exit from unemployment and the employment prospects of the long-term unemployed. The experimental data used in this investigation allowed us to study the effect of the restart interview on a treatment group (who received the interview as usual) relative to a control group (who were not given the interview). The statistical analysis proceeded by distinguishing between exit from unemployment to any kind of job, including part-time and temporary jobs (any job), and exit to a full-time job lasting at least 3 months (stable job). Nonparametric analysis of these distinct definitions of the exit state revealed some crucial differences in the effect of the restart interview. We found that the control and treatment groups were clearly distinct in terms of the Kaplan–Meier survivor function for the any job definition of the exit state but not for the stable job definition of the exit state. Further nonparametric analysis of the exact intervals of time over which the treatment and control groups were distinct using the test suggested by Dinse *et al.* (1993) supported this view.

The econometric model specified in this paper included a Cox proportional hazards model which incorporated a nonparametric base-line hazard and a parametric form of unobserved heterogeneity. The estimation results showed that the regressor for control group status was statistically significant for the any job exit but not for the stable job exit. This supports the finding that the restart interview seems to be ineffective in helping the long-term unemployed to gain a permanent regular job. The econometric results also suggest that unobserved heterogeneity is present in the analysis of the duration to the stable job exit. Although controlling for unobserved heterogeneity had little effect on the parameter estimates, the irregular nature of the base-line hazard highlighted the importance of the semiparametric approach.

The main policy relevant finding of this paper is that although the restart programme has succeeded in moving people off the unemployment register to any job (which includes short-term, temporary and part-time jobs) it has not been so successful in terms of moving people into stable jobs and hence ending their dependence on welfare payments. It is possible that our statistical analysis is too limited in that it looks only at the distinction between short-term and part-time jobs and those which are full time lasting 3 months or more. It could be that, for many who have been unemployed for a long time, finding any job, even a short-term temporary job, could act as a first step in helping them back to a regular job in the long run. This question provides an important direction for future research.

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APPENDIX A: DESCRIPTION OF VARIABLES†

Inner city	Inner city identifier (1≡inner city)
Sex	Sex (1≡male; 0≡female)
Past unemployment	Proportion of the individual's working life since 1982 which was spent in unemployment, calculated from government data from the Joint Unemployment and Vacancies Operating System
Any job	Continued duration of current unemployment spell (months) beyond the sample date and ending in an exit to any job: this variable is calculated from self-reported data and months spent in training before exiting are netted out; individuals who exit from the labour force before finding a job are treated as censored at that date
Stable job	Continued duration of current unemployment spell (months) beyond the sample date and ending in an exit to a full-time job lasting at least 3 months: this variable is calculated from self-reported data and months spent in training before exiting are netted out; individuals who exit from the labour force before finding a job are treated as censored at that date
Dependent kids	Total number of dependent kids (≤16)

Toddlers	Total number of toddlers (⩽ 5)
Local unemployment	For each 1978 job centre travel-to-work area this variable measures the decline in unemployment between 1988 and 1990 (average local unemployment in 1988 – average local unemployment in 1990)
Control	Restart subgroup (1 ≡ control)
Driver	Do you hold a current driver's licence (1 ≡ yes)?
Married	Married (1 ≡ yes)
Divorced	Divorced/separated/widowed (1 ≡ yes)
Race	Race of respondent (1 ≡ white)
Education	Any academic or technical qualification (1 ≡ yes)?
Active partner	Partner working (full or part time) (1 ≡ yes)
Benefit entitlement	Predicted benefit entitlement
Rent house	Rents a house from a housing association etc. (privately) (1 ≡ yes)
Local authority house	Rents accommodation from a local authority (1 ≡ yes)
Own house	Own or buying a house on a mortgage
Other house	Other form of accommodation other than home owner, renting from a local authority or renting privately (e.g. living rent free or squatting) (1 ≡ yes)
Age25	Dummy variable taking the value 1 if the individual is between 25 and 35 years old; a similar definition applies to Age35 and Age45 whereas Age55 indicates individuals 55 years old or over

†The reference groups for the age and housing dummy variables used in Table 1 were whether the individual was aged less than 25 years and whether or not the individual owned their own house.

TABLE 3
Summary statistics

Variable	*Treatment group*		*Control group*	
	Mean	*Standard error*	*Mean*	*Standard error*
Benefit entitlement	43.57	19.20	43.42	18.34
Sex	0.673	0.469	0.685	0.465
Age25	0.271	0.445	0.248	0.433
Age35	0.162	0.368	0.196	0.398
Age45	0.128	0.334	0.122	0.328
Age55	0.103	0.304	0.108	0.311
Married	0.466	0.498	0.451	0.498
Divorced	0.100	0.301	0.136	0.344
Dependent kids	0.515	0.963	0.524	1.08
Toddlers	0.287	0.630	0.266	0.648
Local unemployment	0.349	0.054	0.348	0.052
Inner city	0.179	0.383	0.203	0.403
Race	0.980	0.141	0.972	0.165
Education	0.558	0.497	0.563	0.497
Driver	0.496	0.500	0.524	0.500
Local authority house	0.238	0.426	0.258	0.439
Rent house	0.096	0.295	0.119	0.324
Other house	0.013	0.115	0.021	0.144
Past unemployment	0.392	0.271	0.397	0.268
Active partner	0.221	0.415	0.248	0.433
N	4266		286	

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